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THE JOURNAL OF PHILOSOPHY

THE MEANING OF MEANING IN HOLLINGWORTH'S *THE PSYCHOLOGY OF THOUGHT*

PROFESSOR H. L. HOLLINGWORTH has placed the reviewer of his analysis of meaning in a very advantageous position. If the reviewer's treatment is woefully inadequate, or even if it presents merely irrelevant details, yet Professor Hollingworth must remain silent and agreeable. Has he not himself defined meaning in precisely such terms? And will he refuse to live by his definition when it comes to a meaningful description of his own position? Thus we may put on a bold front as we go forth on our hazardous way. But even a warrior armed with such a sword must be watchful, and in the battle of wits beware lest the approach of drowsiness or the far-fetched substitutions and puns of that dangerously wasteful ogre, sleep, overpower him. So we must not catch even a catnap on our expedition, lest in our dreams the nap turn into a trap and catch us.

So, leaving fantasy-formation aside, let us start with a provisional, general definition of meaning. For Hollingworth, meaning is a type of behavior where a present stimulus, which has previously functioned as a part in a larger stimulus, calls forth the same response that the larger, total stimulus previously gave rise to. This is what Hollingworth calls "redintegrative sequence." "A complex antecedent, *ABCD*, instigates a consequent, *XYZ*. . . . Thereafter, if it occurs that the consequent *XYZ*, or one belonging to the same class, is instigated by detail *A*, or a detail belonging in the same class, by virtue of the historic participation of *A* in the situation *ABCD*, we have a case of meaning. The fact of meaning is the determination of *XYZ* by the previous context of *A*. Meaning is thus a function, a dynamic relation; it consists in the part-whole efficacy of *A*. Meaning is the fact of redintegrative sequence" (pp. 213–214).

Let us look somewhat more closely at this definition. In the first place, meaning is one kind of sequence. There are, Hollingworth tells us, two kinds of sequence. One is the sequence of natural events, the other is the redintegrative sequence. In the former, the consequent arises only when all or a certain minimum of necessary aspects of the antecedent are present; in the latter, any detail of the antecedent may set off the total consequent. Natural se-

quences are thus quite fixed, but sequences of meaning present the characteristic of wide variability; ". . . the 'physical' event follows certain particular details of a context, and them only, whereas the animal and human adjustment [the redintegrative sequence] is to the total antecedent context, any detail of which may thereafter be effective" (p. 203). Hollingworth does not say that meaning is restricted to animal and human behavior (to *S–R* units), but this seems in general to be his view. We should, however, point out in passing that not all human behavior is meaningful; some of it takes place on the natural sequence level.

Meaning, then, is a form of behavior characterized by a substitution of stimuli. But these stimuli must not be viewed as merely external and foreign to each other. Rather, the original stimulus must be a complex whole and the substitute some part thereof. Let us diagram it:

(*S*)	(*R*)
ABCD	*XYZ*
A	*XYZ*
B	*XYZ*
(etc.)	

This differs from the conditioned reflex in that the first response is to the total stimulus, and the stimuli that later come to be substitutes do not gain their efficacy by occurring simultaneously with some other, primary or proper stimulus, but by the fact that they were originally a real and necessary part of the stimulus. Furthermore, redintegration is not restricted to what may properly be called reflexes. Nor does it depend upon repetition, but often functions effectively the first time a partial stimulus reappears.

The reaction in a case of meaning need not be overt, muscular adjustment, though it may be. It often takes the form of words, attitudes, symbols, reports, or conscious experiences. Hollingworth presents three levels of meaning on the basis of the type of response involved. "1. On the postural level meaning is concerned merely with our conduct, our overt behavior and actions. . . . I understand or know the meaning of a carburetor, a lock, a stove, when and in so far as I can handle it" (p. 228). "2. On the autonomic or affective level I know meaning when I get the appropriate feelings, attitudes, and emotions" (p. 229). "3. Intellectual or cognitive meaning is the presence of an appropriate context of concepts, images, references, relations, notions, ideas, associative tendencies, and, in many cases, of appropriate naming responses and of varied secondary meanings" (p. 229).

In general, Hollingworth is consistent in applying the part-whole relation to stimuli only, but in a very interesting passage

(pp. 224–228) he finds himself brought face to face with the question whether this relation is not true of reactions, also. That is, may not a later, partial reaction act as a substitute for an earlier, more elaborate, total response to the same stimulus? "In this way the constant or repeated stimulus *S* leads originally to reactions *A–B–C–D*, but finally it leads only and directly to *D*" (p. 225). Hollingworth seems loath to call this, as it stands, meaning. However, he does present a tentative shift of the part-whole efficacy from the reaction side to the stimulus side. I am not sure that I understand him, but he seems to argue thus: Let the omitted parts of the reaction be treated as a part of the organism, a modification of the reagent. Then treat at least so much of the organism as a part of the original, total stimulus. Now let this part of the stimulus drop out, and *S* functions redintegratively for the whole. The following may serve as a diagram, (1) being the ordinary view of a telescoped response and (2) being Hollingworth's interpretation:

I see no objection to this contortion. It seems to prove, however, not so much that all part-whole efficacy is restricted to stimuli as that the distinction between stimulus and response is quite arbitrary and can be shifted at will in any given case.

So far, we have stressed the idea that meaning is a form of behavior, and hence must be dealt with in terms of stimuli and responses. But we must now emphasize that meaning is a particular type of *S–R* relation, one involving redintegration. The present, partial stimulus functions vicariously for its former whole. Let us inquire into this vicariousness.

In meaning, a part functions for a whole. Sometimes Hollingworth seems content to leave the matter there, and not add that the part functions for the whole by instigating some further reaction. In fact, we are at times left with the impression that meaning may be simply the reference a part bears to its whole, or to other parts in a common whole. "The redintegrative account of meaning applies even to natural signs or natural laws. Three equal angles *mean* three equal sides, in a triangle. This, we may suppose, is a case of 'logical implication,' or 'geometrical necessity,' or 'matter of fact,' or 'association of ideas,' as one may prefer. But no matter what vocabulary is employed, the simple fact is that 'three equal sides' is *part of a total* which includes 'three equal angles'" (p. 230). "In this way fire means smoke, sand in the sugar means broken teeth, a north wind means cold weather" (p. 231). "When

one end of a tiltboard is elevated, the other is depressed. The elevation becomes a sign of the depression. . . . The syllogism in this case rests merely on the nature of a rigid body, and inference from one detail is only taking the one as the sign of the other, or of the whole'' (pp. 231–232).

The sign or symbol, then, is always incomplete. Often it is a mere detail, quite irrelevant to the essence of the whole that it means. Chance association, superficial resemblance, are bases enough for its becoming a surrogate. ''What we call traces, or images, or thoughts, of objects and events are distinguishable from the originals which they *mean* only by their incompleteness'' (pp. 235–236). ''It must, however, be noted that the detail present in thought may not be a very conspicuous detail of the immediate perception of the historic event. It may instead be a trivial and accidental detail, even what we call an 'associated' detail, such as a word, a gesture, an attitude, a specific feeling, an imaged article of clothing. But any and all of these were part of the original event or situation'' (p. 237).

Nevertheless, our author seems to realize that symbols may occasionally be true wholes themselves, as complex as, or even more complex than, the wholes whose place they take. Here the vicarious nature of meaning is not so much a part-whole relation as a confusion or substitution of two wholes on the basis of partial identity. ''Within the content of the drowsiness fusion or hallucination, a present impression, a perseverative tendency, or even a pure memory element often substitutes itself for some other datum, whose rôle it fills in the perceived composition of the hallucination. The various types of substitution or the varieties of surrogate are interesting because, however different their origin, they are all capable of assuming a vicarious or substitute rôle'' (p. 55). ''For rough descriptive purposes the phenomenon we here called 'substitution' might instead be indicated by the simpler term 'confusion.' Situations, objects, and facts, having some element or feature in common, are confused with one another, much as children mistakenly identify objects possessing some common element. . . . Not only are situations and events 'confused'; they also, as a result of this confusion, take the place of one another in the forward movement of association and action that constitutes thought'' (p. 57). In speaking of the meaning of a dream where the subject's tossing in bed became a process of sewing seams in a garment, Hollingworth says, ''Here it seems clear that the substitution of one experience for another, because of their several points of agreement, was the basis of the dream'' (p. 95). This might be expressed in the following way: a complex experience, *MCNO*, becomes a symbol or substitute for another complex experience, *ABCD*, on the basis of the common feature

C. This is certainly a case of vicariousness, but whether it could also be termed redintegration might be questioned.

Before leaving this matter of vicariousness in meaning, we should touch upon Professor Hollingworth's treatment of images. He holds that there is no truly imageless thought. Yet the doctrine of imageless ideas or meanings was the natural outcome of a false view of images in their relation to meaning. This false view held that an image, to be a symbol, must duplicate or copy the original experience to which it refers. Hollingworth insists that it need not, that it usually does not. All that is necessary is that the image, no matter how irrelevant, function vicariously for that which it means, on the basis that it has previously been a part of this latter (although, perhaps, a very insignificant part). "Thoughts are not copies of situations, but fragments of them" (p. 208).

In fact, symbols do not need to be images. The essence of a symbol is its vicarious functioning, and not its psychic reduplication of the original. Thus the symbol may be *any* kind of event or experience that so functions. "Thoughts are events of any sort whatsoever that function for larger antecedent contexts; they are not mysterious phenomena belonging to a supernatural realm [as images have often been conceived]" (p. 208). "Revived processes of almost any sort whatever, or even present incoming impressions, may vicariously function as standard-bearers for any intellectual exploration that may be in progress at the time. Thought may then never really be imageless, nor will it necessarily move in terms of a rigid photographic sensationalism. Thought must have garments, it is true, but it need not rely on its own providence. It is amply served by the charity of the passing moment,—by the vicarious functioning of irrelevant sense content" (p. 148).

In his discussion of the pure relations of imageless thought, Hollingworth is led to treat meaning from the standpoint of relations and terms. This leads to another, and perhaps not wholly coherent, aspect of his meaning of meaning. In general, he is controlled by the assumption of the externality of terms and relations. Sometimes it would seem that the meaning is the relation (which is non-sensory, yet is an immediate, natural event), whereas the symbol is the two poles which bear the relation, and which may be almost any sensory or imaginal experiences; ". . . the vehicle of a waking meaning, the two poles of a relation, may be fragmentary, transitory, and only remotely relevant . . . " (p. 144). "Most, if not all of our thinking, is *of* relations, *by means of* related symbols" (p. 151). But it is also true that terms may be the symbols for other terms, and relations for other relations. "Now the observations show that relations may function vicariously, even as do the contents or terms"

(p. 151). "In some cases the symbolic function of relations is, through daily habits, become so inveterate that relations of one sort are represented by other relations as automatically as they are named. Even the naming of relations often suggests the basis of transfer. Pitch relations, thus, are named as higher or lower, and are usually spatially represented" (p. 162). "But to conclude that the originally perceived relations are represented by the sensory or imaginal details of the present instant is a grotesque fantasy. It is the new primary relations that mean the original primaries, just as the 'revived' imaginal contents or the new kinaesthetic sensations may represent the original primary objects or qualities" (p. 156).

This last quotation reveals a fresh idea. Relations, whether original or symbolic, are always primary, fresh, brand new. They are never revived. Terms, however, may be secondary, stale, old, revived. But, we may ask, are not relations as well as terms always both fresh and stale? Along with this peculiar sorting of the fresh from the stale is another interesting division. Hollingworth asserts that both terms and relations may function *either* merely for themselves *or* vicariously. Their vicarious functioning is an addition to their immediate nature, which addition does not change them. The question arises whether they do not always function both for themselves and vicariously. But here we are getting away from description and into criticism.

By way of a final descriptive analysis, which is on the verge of criticism and so will serve as a transition to our next section, let us indicate Hollingworth's attitude toward universals. Meanings are natural events or sequences of natural events. They are thus always particular. In so far as meaning involves identity (e.g., of the response to the original stimulus with the response to the present redintegrating element), it does not depend upon any universal, but simply upon membership in one class. "Briefly, this problem [the problem of identity] must be approached not through the way of 'universals,' but by the aid of the doctrine of 'classes.' For our present purpose such expressions as 'the same event,' the 'same detail,' the 'same response' can be more accurately rendered 'a detail, event, or response belonging to the same class,' or even 'details, events, or responses indistinguishable from others for a given purpose'" (p. 245). "The essential point is that the psychology of thought need not become involved in the riddle of 'universals,' as propounded by the idealistic logicians. . . . Identity is only absence of change, and change is always a succession of particulars" (p. 246).

In summary, then, Hollingworth stresses the following charac-

teristics of meaning: it is a form of behavior; in it, parts function for previous wholes; often it appears to be a substitution on the basis of a partial identity; it is not a copying by means of images; any type of experience may become a symbol providing it function vicariously; relations, which are always fresh, may be borne by almost any class of sensory or imaginal terms or poles; meanings are always particular and natural, and do not involve any reference to universals. This is far from exhaustive, and we can hardly hope it will accord with Professor Hollingworth's evaluation of what is fundamental. However, armed with the good sword that we previously fashioned, let us hope that we have protection against attack from the rear; and in this hope, let us press forward to the more dangerous undertaking of criticism.

II

Professor Hollingworth's discussion of meaning is surely stimulating and provocative of thought. It is overflowing with startling-places for investigation and rumination. Of the many lines of questioning it has suggested to me, I wish to set down three that seem to be fairly central and representative.

Every meaning presents us with an identity that somehow persists through change or retains its integrity through a series of manifestations. This identity is usually designated as the meant or the signified. So the first question to interest us is, "Where does Hollingworth locate the identity essential to meaning?" There seems to be two possibilities: the "same response" which continues to be elicited by the vicarious, partial stimuli, and the total, original stimulus, whose maternal instinct bids it enfold all its lesser offspring to its own capacious bosom. But is either of these really sufficient?

The "same response" seems to distill itself into a mere repetition of similars, when warmed by the light of close scrutiny. Besides, we are explicitly told that its identity is merely the sameness ascribable to many particulars belonging to one class. In short, each appearance of the response seems to present us a nature all tied up and sealed, complete in itself. As always happens when identity is reduced to repetition and similarity, we find here the tendency to treat sameness as really not uniting at all, but as simply a character of each particular. If this analysis of Hollingworth's "same response" is correct, then the latter does not fulfill the requirement we have seen meaning always presents, for it gives us only a specious identity.

But how about the total, historic stimulus or event that so graciously passes on its nature to its leaner offspring? Is not this the

identity we have seen meaning demands? If at first we are hopeful, let us take heed quickly from our author's own warning. He bids us eschew universals. All events are particular. Therefore this one must be. Once gone, *it* never returns. Perhaps its effects live on after it; perhaps it lends efficacy to its own dismembered parts; but its own life blood is gone in the one pulse that constitutes its existence. This surely is sad. The mother rules her children with iron hand (Hollingworth would seemingly give them no leeway), but her extermination is a prerequisite for her rule. I wonder if Professor Hollingworth is not really regressing here to his pre-adolescent religious training? Has he not endowed this mother with another, ephemeral existence, so that, though dead, yet doth she live? 'Perhaps her identity has simply changed from coarse, restricted, sensuous matter to refined, out-reaching, invisible ectoplasm. To put the matter nakedly; has not Hollingworth got in his original, total stimulus both a truly persistent, change-including identity and a merely specific event, completely destroyed by the first appearance of change? But we must take him at his word rather than guess at his intent. When the vicarious or substitute stimulus is functioning, the original one is absent, has ceased to be. And surely, no specific, previous event can give us the identity that meaning demands. Consider the word "meaning." Does it function as a substitute for (does it mean) an original, total, specific event or stimulus? If so, point it out. What was this original? The embarrassment is not due to forgetfulness. Rather, it arises from the fact that every word refers to a common nature (not to a merely particular event), and to a persistent identity (not to a merely previous situation). What is meant, what is substituted for in meaning, is never a mere specific. And since Hollingworth tells us that his original, total stimulus is a particular event, we can not find satisfaction in this direction.

But Professor Hollingworth is clamoring to defend himself. "I agree that meaning always involves and is based upon identity. But that identity is to be found neither in the 'same response' alone nor in the original stimulus alone. It is to be found in the fact that later, partial stimuli function for the original one in an identical way, by calling forth the response which the original stimulus called forth. The identity is one of function." This is fine. It kills forever that insidious foe of thought, the static entity. But let us inquire into this efficacious deed we have attributed to Professor Hollingworth. Does it mean that the calling-forth and the response are distinguishable? Evidently it does. Then let us question the calling-forth as we previously did the response. Can one calling-forth be the same as another? The two occur at different times,

are consequent to different stimuli, etc. I take it, by analogy, that our verdict here, as before, must be not "the same," but "similar," or "belonging to the same class." In short, we have again a series of particulars each of which is destroyed by the arrival of change. And since we have been told that identity means the absence of changing particulars, our conclusion is easily drawn. Meaning is left without any real identity, and so is meaningless.

Let us start another line of inquiry. Besides a persistent identity (the meant), meaning always involves a more or less changing symbol, which means or refers. It will be interesting to question Hollingworth as to the nature of this symbol and its relation to that which is signified or to that for which it is substituted. Professor Hollingworth tells us that the symbol is always a part functioning for a whole. Now, this may mean one of two things (and I think Hollingworth usually means both without distinguishing them). It may mean that, so far as the part does function for the whole, it *is* that whole, but the whole peculiarly summed up in one of its aspects. Or it may mean that the part simply takes the place of (and thus excludes), substitutes for, or functions in lieu of the whole. In harmony with his tendencies toward specificism, Hollingworth also tends, I feel, to favor this last emphasis. Many questions spring up. Can the present part be called a part of the whole which is now past? Rather, is it not simply *similar* to a feature which *was* a part of that whole? If the original whole has really ceased to be, has not every part perished with it? If we are asked whether parts can not retain their identity even if the whole be destroyed, then we must obviously counter by asking whether their identity in other respects has not been confused with their identity as parts of this whole that has ceased. In short, can parts function independently of their wholes (even though that function be the intimate one of vicarious substitution) and still, in that independent capacity, remain parts of these wholes? Has not Hollingworth made a vital mistake here, and confused vicarious substitution of parts for wholes with the vicarious immanence of the whole in every part of itself? Surely it is this latter that symbolization demands, and upon which even the former depends, though confusedly. When we say, "The symbol is a part of the thing meant," we must add, "but it is also and thereby the whole." Hollingworth's difficulty is that he has made the identity of the whole external to all change, and thus he must isolate it in a particular past time. Every occurrence of a part-symbol must be in a new time, and so must be cut off from the whole. But may we not conceive of the whole as including the changing process of symbolization of it, and thus itself growing while still retaining its identity? Does not the whole truly

share and, in fact, live its life in the fortunes of its parts? Has not Hollingworth really destroyed the vital nature of the relation of symbol to the symbolized by implying an externalism of the two aspects involved?

We have come now to our widest and most speculative of questionings. Hollingworth says that meaning is one form of sequence, to be contrasted with another. Or, to state it differently, but still in harmony with his thought, meaning is one type of behavior. We have already agreed that meaning always involves change, but there might be a question whether "sequence" or "behavior" would intimate the other aspect of meaning that is always involved, namely, that of permanence and identity. But passing by this line of thought because it comes too close to our previous ruminating, let us ask another sort of question. Can meaning be *restricted* to some *one form* of sequence or behavior? Does not every change belong in the realm of meaning? What would meaningless change be? Would it be simply immediate experience in all its concrete complexity, with no economizing functioning of the partial for the complete? If so, we seem to be in trouble. For *all* reality so far as we can discover is always pointing beyond itself to further, unexhausted detail. Can we restrict meaning to some one form of behavior without distinguishing this type of behavior from other reality and claiming meaning for this distinction? Does meaning occur as a distinct sort of event within a reality including many other sorts of things, or is it simply an aspect of all that is real, but an aspect we abstract for inspection? It is only fair to point out that Hollingworth does hint at this last intimation when he ascribes meaning to what he calls natural sequences, which he has contrasted with redintegrative sequences. If he were to push this thought, no doubt his general emphasis would become profoundly changed.

We have purposefully omitted many enticing questions because of the desire to call attention to what we conceived to be fairly basic. These basic questions were: Where does Hollingworth give us the identity that includes change and that is usually designated by the meant? What is the ultimate relation of the changing, partial symbol to its total, permanent meaning? Can meaning be restricted to one sort of event, or is it an abstracted aspect of all the real?

All this suggests our own view. Much that Hollingworth stresses is vital. Meaning is a part-whole relationship, where the part functions vicariously for the whole. The part, which functions as a symbol, can be almost any feature of the whole, and often is some quite unimportant detail. It need not be an image in the traditional sense, and is practically never a little or vague duplicate

of the whole that is meant. But we must deviate from Hollingworth in certain other vital respects. The whole for which the partial feature functions as a symbol is never a merely particular event occurring all at once at a time previous to the functioning of the symbol. It is rather a "universal" that grows in the process of reference to it. It is universal not in the sense of being of another essence from change and difference, but in that it includes these and unites them by transcending them in its own identity. It is universal because it refuses to let temporal, spatial, or other differences split it up into a mere collection of particulars. Furthermore, the part that functions vicariously for the whole presents that whole immanently in its own nature. It is no mere substitute by virtue of filial duty or family resemblance. It *is* the whole. Yet we must be careful here. It is not the whole in all the latter's inexhaustible concreteness. What we must recognize is that completeness and partialness are not two separable characters of the world. They are warp and woof of the texture of things, and it is their very unity and interdependence which is the essence of meaning and the paradoxical core of human knowledge. They are the flickering, entangled light and shade that compose the ever-fleeing, never-quite-grasped mystery of this finite life of ours.

EVERETT W. HALL.

LAWRENCE COLLEGE.

MEDICINE AND SCIENCE [1]

WHERE the study of disease belongs in a scheme of knowledge is not a new inquiry. It is my object to analyze the situation of medicine; to show how it deals with unique phenomena; to show why it is entitled, not by courtesy, but in the nature of things, to its high status as a natural science; to illustrate the justice of this contention in so far as this can be established in such terms, by comparing its structure with that of other sciences; and finally to indicate the variety of opportunity this many-sided discipline affords.

Distinguished leaders in Germany, to which the habit of three generations has accustomed us to look for light and leading, have recently been engaged in a polemic often pessimistic on the aims and organization of medical thought and practice. Many of their observations are germane to this discussion, but for the moment I prefer to lay aside certain of the issues they have raised; they are issues of organization, of the psychology of patient and physicians, of the

[1] Based on an address delivered at the Opening of the University Clinics and New Medical Laboratories of the University of Chicago, October 21, 1927.

relative significance of practice and learning. I find it imperative rather to face problems more fundamental. I am more concerned with medicine itself and its analysis than with how such issues should be met and who is to meet them. These are secondary questions. They will be answered when there is agreement on what is essential in the situation. The problem can be defined by quoting statements by Sauerbruch (*Naturwissenschaften,* 1926, xiv, 1081–1090) which reveal two underlying opposing motives in medicine. The first statement declares that there is native in us the impulse to help the sick ("Der Trieb den Kranken Menschen zu helfen ist uns angeboren"); and the second that we experience a compulsion to explain life and its phenomena ("Der Drang, das Leben mit seinen Erscheinungen zu erklären"). "The moment," he says, "in which this second phase of medical interest becomes conscious and operative, a tendency antagonistic to the first can be recognized as having developed." ("In dem Moment wo dieses zweite Element ärztlicher Tätigkeit sich durchdringt zeigt sich schon sein Gegensatz zum Ersten.")

We shall get no further with this *Gegensatz,* this antinomy of Sauerbruch, until we make an effort to define what we mean by medicine and what by science. I do not mean to imply that without making conscious definitions, significant direction in research or in practice in any calling may not take place. In the pages of history there is ample evidence to show how often development has occurred without precise definition. But may there not be another kind of procedure? Does not the current movement in medicine suggest that a systematic method may be employed? I mean, at all events, to employ it, in order to explore what experience and tradition the history of science holds out to medicine.

For medicine the hope has been expressed before now that it may become a science. But many doubt that this is a possibility. The statement is made that the content of its interest is too diverse, that it is subject too much to accident, too much to the source of interest of the moment; we are told that one disease disappears and is conquered only to make way for another and that its subject-matter shifts too much to expect to constitute it a science. This surely is the thought principally of those who think of medicine chiefly in terms of infectious diseases. But others I find go further still and maintain that this content can not lend itself to treatment proper to a science.

So far, a claim to regard medicine as a science, in the sense in which other disciplines are presumed properly to be so regarded, has scarcely been made. If medicine is not a science, but only one of the crafts, a calling which thrives by applying to its own subject-

matter theoretical knowledge obtained outside its own practice, and practical knowledge the result borne of the successful repetition of well tested acts, there is no use in the elaboration of complete intellectual machinery to carry out its purpose. All that is required is practitioners. What the training of these should be is a matter about which one need not stop to inquire; for trades have always, through their guilds, found the method by means of which their craftsmen have been furnished with sufficient instruction. But this is not the situation in medicine; medicine has accumulated theoretical knowledge of its own and this has had its origin in age-long and varying experience. These possessions, as we shall see, are the occasion for developing a science.

I adopt Sarton's definition of science as "systematized human knowledge." But there need be less concern on the score of a definition of Science than on that of the definition of *a* science. I intend later to attempt to analyze what may be meant by this phrase. By the term "medicine" I mean the discipline which is engaged in recognizing, in distinguishing, and in studying diseases; the subject-matter of medicine is the sum total of human ailments. It is devoted to the study of disease in the living, fostered by whatever means may appropriately be employed. It is not coextensive with any method, such as experimental pathology, but utilizes data so obtained, whether analogical or inferential, for its own purposes. It does not necessarily include therapeutics, even though in times past Laennec went so far as to declare that the object of medicine is the cure of disease. It seemed for decades a wise statement. But whether it was so, made little difference so long as the building up of knowledge required, relatively speaking, a simple equipment. Now it is not the fact that the necessary equipment has become complicated that makes the difference; the essence of the matter remains the same. But the very cumbersomeness of the new requirement demands an analysis and a readjustment of the problem. It appears now that medicine is concerned with knowing about disease, a very different thing from making efforts to cure it. Cures have remained largely empirical, except in instances such as the treatment of cretinism and of diabetes, where the method of procedure has had a genuinely rational origin, and perhaps also in the case of certain infectious diseases, in which the nature of the infecting agent was considered in developing immunological procedures. The two should be separated—medicine and therapeutics. The failure to separate them has confused an issue; so long as therapeutics remains inseparably bound, conceptually, with medicine, there is no escape from the conclusion that medicine is and must be regarded as an applied science. But in the separation medicine becomes what physiology or

any other biological science is—a discipline affording an opportunity simply to investgiate arrangements and processes of diseased living matter. This search does not necessarily involve the notion of consequent practical action. The situation is exactly analagous to that in the physical sciences; for there a study is not necessarily antecedent to application, to an attempt to interfere with a natural process. Many illustrations may be cited. The names of Kepler, Galilei, and Newton; of Descartes and Leibniz; of Lavoisier and Dalton, of Volta and Maxwell, occur at once in connection with astronomy and mechanics, with mathematics, with chemistry, and with electricity. Technical application was not the object of these discoveries. The consequence, the applied action of discoveries in physics, is recognized at once to be engineering. Invention, engineering is the application of the physical sciences. This analysis implies nothing invidious; it purports to be merely a statement of the facts. The relation between medicine and therapeutics is comparable. Medicine is the study of diseases, regarded as biological systems; therapeutics an attempt to utilize that knowledge for the purpose of interfering with or rectifying abnormal courses. That there should be systematic research in therapeutics even as there is in the study of disease, admits of no doubt. Nor is there the least reason for denying or doubting the fact that in discovery the applied may, and sometimes does, precede the underlying science. But to establish this sort of priority is not the reason for urging this separation; its object is clearness in thinking—as will appear later. In point of fact one may doubt whether strictly rational therapeutics *can* develop far until knowledge in medicine proceeds beyond its present state.

Still another argument is made against including medicine among the genuine natural sciences. This argument states that the motive for studying diseases is humanitarian and that this motive is not one which has occasioned an interest in nature like those which its other phases have elicited. It states also that whereas other sciences concern themselves with the normal, medicine concerns itself with the pathological. But such arguments scarcely have an inherent justification. They rest on the notion that there is an invidious distinction between an interest in man, especially diseased man, and all other natural objects. The pendulum has swung far indeed, from its position a hundred years ago, when it was claimed that the prime object of human interest, perhaps an unjustifiable overstatement, was man. A science does not depend on whether the motive for study is humanitarian nor need it stop to inquire whether the subject-matter is pathological. It would, indeed, be difficult to draw a dividing line which limits the domain of a humanitarian interest to

one of its sides; in a sense all science is humanitarian. If by humanitarian is meant sentimental, that indicates merely a more immediate and pressing interest and need not prejudice either the ultimate object of the study or the seriousness of the undertaking. If something less serious is meant it need scarcely for that reason deflect any one from his legitimate scholarly pursuits. To insist that the content is pathological rather than normal is to misplace an emphasis, for surely both phases of life are natural. That it is studied early in the development of biological science, when every means at one's disposal is eagerly grasped to further the solution of such problems, argues only its urgency, not its irrelevance.

If medicine is to be a science and is to be so fostered it must in all probability behave as sciences in general do. Huxley has put the matter in this form: "The object (of physical science) is the discovery of the rational order which pervades the universe; the method consists of observation and experiment (which is observation under artificial conditions) for the determination of the facts of Nature; of inductive and deductive reasoning for the discovery of their mutual relations and connection. The various branches of physical science differ in the extent to which, at any given moment of their history, observation on the one hand, or ratiocination on the other, is their more obvious feature, but in no other way; and nothing can be more incorrect than the assumption one sometimes meets with, that physics has one method, chemistry another, and biology a third" (*Essays*, Vol. I, p. 60). Huxley has been at pains to show that "physical science is one and indivisible." This statement prejudges the matter, however, for we have yet to show whether medicine may belong in this company of the sciences. No doubt there is general agreement on what the object and the method in the natural sciences are. All of them examine the phenomena which comprise their subject-matter, analyze them, make deductions from the analyses, and proceed to generalizations from these. They have, in fact, in recent times followed the ways of observation, of classification, and of deductive and inductive inference.

It has been my object so far to indicate roughly the background for considering this problem of the place of medicine in the Fraternity of the Sciences. The greater difficulty is still, however, unexplored and concerns the troublesome matter of defining what is meant by *a* science.

Huxley spoke of the object in science as being "the discovery of the rational order which pervades the universe." At the basis of any science is curiosity as to natural phenomena. In order to apply the methods of science to them it is almost indispensable to separate certain phenomena obviously related into groups or classes on ac-

count of certain superficial resemblances. A group so constituted may then be called a science were it not that objection would at once be raised that a claim such as this might serve as the adequate basis for a science of alchemy, or astrology, or a science of certain forms of psychology. Apparently additional criteria are needed before a class of related phenomena are believed to constitute a science. The hypothesis which underlies the segregation must be tested. As sophistication develops, the tests become more severe. Superficial resemblances give way to others more profound; Koch's rules for identifying micro-organisms as the agents of disease become only partially adequate. These rules require, it will be remembered, that bacteria should first be isolated, then grown artificially outside the animal body, and finally be injected in animals in order there to reproduce the original disease. In other directions, concrete descriptions give place to statements more general and more abstract; the more abstract and general these become, the more do they purport to be universal and to serve as the basis of prediction.

Through what cognate stages, then, must we pass before medicine may be spoken of as a science? The first must clearly be that the phenomena of diseases are recognizable in nature. Diseases are in point of fact unique phenomena in nature; they do not exist as independent entities, nor are they manifest in normal individuals. They are, on the contrary, inextricably associated with organisms which become quite new beings due to the presence of disease. Of course, a diseased individual does not lose his form, nor are his general activities unrecognizably altered by the presence of disease, but in many quite subtle ways the mechanism of his organization alters. The altered state is seen very clearly in infectious disease when the mechanism of immunity comes into play. It is seen also in hormonal diseases such as exophthalmic goitre, where the principal malfunction brings in its train derangement of so many other mechanisms of the body. It is more conspicuously seen in the alterations during circulatory failure in response to which practically all the processes in the body take on new phases. These illustrations suffice to indicate the uniqueness of living organisms under the influence of disease. This uniqueness is crucial for science, for if the phenomena are unique they must be regarded as the bases of medical science, just as bacteria, rocks, molecules, and fossils are bases in the sciences in which they in their various forms are the objects of inquiry.

Disease phenomena must, moreover, give the appearance of being *prima facie* related. An outstanding difficulty has been the fact that their interrelations are not immediately obvious. In a study of Linnaeus and of the older nosologies the reasons appear at once.

The endless catalogue of characteristics suggests no coherent pattern. Before infectious diseases could be grouped similarities must be shown to exist, communicability—fever—infectious agent. Perfection in classification could come only after the institution of these and similar criteria. The discovery of certain of them is within our own memory.

Next must come the assurance that there are methods for dealing with them. And then comes a crucial question: Are they to be dealt with in the domain of some discipline already in existence, such as experimental physiology and experimental pathology, or are they to be treated by themselves alone? Are they to be studied as diseases, or as phases of pathology, as the phrase goes in England, or as pathological physiology, as it does in Germany? Elsewhere the suggestion has been made that the study should proceed as a phase of physiology, and finally has come the idea, notably in America, that they be relegated to a discipline sometimes called research medicine and sometimes experimental medicine.

I have tried to make clear that a disease is a phenomenon that exists in nature, *sui generis*. By the same token, its study does not lie in the domain of any of the other disciplines which have developed or have heretofore been associated in medical schools; I refer to anatomy, embryology, physiology in the proper sense, chemistry or certain branches of physics, and to any hybrid of them. Obviously the phenomena of diseases, no matter what interrelations exist among the several kinds, are not the proper concern of any of these disciplines. As a matter of fact, the history of medicine since the Renaissance has shown plentifully that whenever the approach to an understanding of disease is made by scholars trained primarily in other pursuits of knowledge and animated primarily by their interest in these, the result, so far as understanding disease is concerned, is disappointing and sometimes grotesque. A similar statement may be made in the case of physiology. There seems little escape from the conclusion that if there are phenomena of disease which can satisfactorily be grouped by themselves, they can not satisfactorily be fitted into any discipline already existing. The only discipline in connection with which an association is conceivable is physiology, but the case against including them here is complete. Physiology has its own problems, great in number and enormous in complexity, and those of disease are not among them. Even if it were possible to view disease or diseases as phases of physiological derangement, the matter would not be settled, for medicine has claims of its own to consideration, the neglect of which leads to quite as much incongruity as did the approach to it of the more mechanical sciences. The case in medicine is the same as that in sciences already well

established and consists as a prerequisite in sensitiveness on the part of observers to changes in the natural phenomena with which they deal. The point is contained in the familiar phrase "chance and the prepared mind." Those of us who in modern times have come into contact with the study of disease when this has been in the hands of chemists and physicists, have become amply aware of the fact that the peculiar nature of disease phenomena themselves interferes with an appreciation of them on the part of individuals who are not primarily trained in their contemplation. No doubt the solution of problems of first importance in relation to disease may originate in disciplines outside of medicine, as in the case, for instance, of Pasteur, and in the future likewise important contributions may be expected to arise in this manner. For the boundaries between the sciences *are* artificial. I am here concerned with the development of medical science, not through chance, but by organization. If there is a satisfactory case to be made for the science of medicine it surely must suggest as a consequence the value of affording to its own votaries the opportunity for investigation.

Disease phenomena then are unique and can be grouped for study in an appropriate discipline. But before the status of a science can be accorded them, it must have become possible to manage them as the natural sciences are managed—there must be available or there must be developed methods appropriate to their study. Physiology has attained this distinction. In point of fact the success of physiology had gone so far at the turn of this century as to embolden its professors to attempt to extend their labors to the study of disease itself, as witness the institution of departments of research or experimental medicine. Here is tacit recognition of an important fact; the belief prevailed that it had become possible to study diseases by rational and precise methods just as had become the practice in physiology. There was available, in short, a methodology for the pursuit of a science of medicine. The methods may not have been, may not be peculiar to, nor have had their origin in, medicine. But that has no ultimate importance. There is, indeed, scarcely a science of which it can be said that its methods were developed exclusively for its own use. In biology, for instance, use is made of physics, chemistry, and mathematics. And conversely, biology, in the case of Galton, has made its own signal contribution to mathematical analysis.

There is a respect in which a distinction between medicine and other sciences may be drawn. The other sciences give the impression of occupying themselves on superficial inspection with a subject-matter that is homogeneous. In the study of diseases, relatedness of the units of interest can scarcely be urged. There is no systematic

relation, for instance, between typhoid fever and Graves' disease, nor between Graves' disease and the heart failure of senescence, nor between heart failure and dementia præcox. How can subjects so unrelated be grouped together? How is it possible to subsume them under the head of a single discipline? They have obviously different origins, different mechanisms, and different natural histories. Does not this diversity interfere with their unification in a single science?

But have these questions real importance in considering the conception that medicine is a science? Consider the other sciences—chemistry, for instance. Can there be greater differences either in content or in the methods employed than in the study of proteins, carbohydrates, fats, and inorganic substances? Each of these is studied independently by methods having no necessary relation to those employed in the pursuit of any one of the others. Others may prefer a different division of chemistry to illustrate this point, but this is a matter of little immediate importance, for on either plan the main contention remains, that familiarity with one aspect of the science need not imply equal familiarity with the others. Is not the same true in physics, where mechanics, heat, sound, and electricity receive separate treatment? But in spite of apparent diversity of appearance the history of physics has exhibited continuously a concern to discover unifying principles in its endeavor to unravel the constitution of matter. It has not changed the object of its inquiry from the beginning until to-day. The Greek natural philosophers very early appreciated the fact that matter must be resolvable into elements. It makes no difference that the number of elements varied, that there were four, five, or ninety-two at different periods, nor does it matter that at one period the elements were thought to be large and at others infinitely small. One of the obvious triumphs of modern physics has consisted in discovering these common elements in appearances which seemed obviously so different. Sight may, of course, not be lost of the fact that however the same they are in their ultimate natures, the first approach to the study of such matters as light and electricity pursued most diverse paths. The direction of the search has always been the same and the general conceptions uniform.

In this respect the study of disease seems at first apparently to suffer a disadvantage. Conceptions of disease disclose no such constancy of view. Until recently there appears also to be wanting a unifying elementary principle comparable to that in physics. It was not, indeed, until the nineteenth century that a unifying principle comparable to elements in physics was hit upon. Müller, and Schwann and Schleiden in the eighteen-thirties, made the great discovery that large and complex organisms were composed of cells.

After this cells in biology became the correlates of atoms in physics or molecules in chemistry. The atomistic view of the organism was introduced, not without misgiving to scholars like E. S. Russell (*Form and Function,* 1916) so far as the meaning and significance of morphology, of form, were concerned. Virchow twenty years later extended this concept when he demonstrated the close relation between diseased bodies and diseased cells. The doctrine of organization, now especially on the threshold of receiving much more attention, undoubtedly is one which also has served to unify thought in biology, and ought perhaps to be placed equal to, or at all events together with, that of cells. I am inclined to think, though, that in the sense of being elementary, meaning thereby reduction to the smallest usable entity, less reliance would now be placed on organization did one not have the security derived from the assurance that the analytical bottom in biology has been reached in possessing the concept of cells. Organization in the sense at all events in which the phrase is now being used is the event of to-day.

Cells in biology, then, are to be regarded as the analogues of atoms in physics. But cells are infinitely more complex than atoms; they are infinitely more complex than molecules, and the laws of mechanics probably do not apply to them in the relatively simple form in which they apply to inorganic matter. That they may apply is possible, but there is reason to believe that this need not be so. It is unnecessary to discuss this subject further than to point out that it involves the question of mechanism and vitalism and perhaps that of emergence. If one goes further and compares the synthesis of the elements in physics with the elements in biology, the difficulties multiply. Even though matter may be simply an aggregate of atoms, it is known that when additive quantitative changes are made, something besides quantitative changes often emerges. In the new synthesis, unanticipated qualitative transformations take place. An uncomplicated instance is to be observed in the difference between the behavior, let us say, of strong and weak solutions of electrolytes. If simple addition and multiplication bring about qualitative change in the inorganic world, how much more complex does the matter become when cell is joined to cell in cell aggregates and when these in turn are multiplied to form organs which in order to function properly must exist in equilibrium in the bodies of complex metazoa.

The meaning of all this seems to be that for the time being or perhaps for all time the effort to investigate living matter with the same degree of simplicity with which non-living matter can be treated, must fail. This situation may be inevitable. Burtt (*The Metaphysical Foundations of Modern Physical Science,* 1925, p. 156)

suggests that ever since 1600, science has consistently been divided into mathematical and non-mathematical aspects. There were Kepler and Galilei on the one hand, and Gilbert and Harvey on the other. Is the inference to be drawn, that living matter can not be regarded and can not be treated as a science in the same way that inorganic matter can? That they are strikingly different is, of course, amply apparent. But that the ability to employ one kind of treatment in the case of one set of phenomena constitutes that set a science, and that treatment of another kind fails to qualify it as one, seems to make a decision depend not so much on the existence of one or the other set of phenomena, or even on one's ability to arrange both sets in classes accessible to intelligent treatment, as on the difference in the methodology employed in the two. Surely the issue can scarcely be decided by defining science as methodology.

It may seem that in these considerations we have wandered far from the question whether the study of diseases may be regarded as a science, but the distance is not so great as it seems at first. It has already been pointed out how various are the forms in which physics and chemistry may be studied and how elementary principles underlie these diverse appearances. A similar analysis was made in considering the case of diseases. Here again classes were found which seem at first to be obviously unrelated; there are hereditary and congenital anomalies, infectious diseases, malfunctions of the organs of the body or metabolic diseases, mechanical derangements, and senescence. In this case the elementary principle was found as Virchow found it, in the pathology of individual cells. That these undergo different disorders when they are affected in diverse ways is what must be inferred from the great variety of derangements which have been described. Being complex, their reactions are multiform. To dragoon them into simplicity and apparent uniformity is probably impossible. Classification of such entities even if complex may be difficult. But classification is antecedent to the possibility of rational, scientific treatment. And this naturally is the more satisfactory, the simpler and the more general the terms. Treatment in simple and general terms is now being met in the inorganic world, but in this sense the attempt to contrive similar methods, even if it does not quite fail, is none the less not yet successful in the case of biology and, of course, in medicine. But even in physics we may recall that Planck (*A Survey of Physics*, 1925, p. 19) has said: "I hope that the above considerations will have sufficed to make it clear that the difference between reversible and irreversible processes lies much deeper than that between mechanical and electrical processes. Therefore this contrast may, with more accuracy, be made the most convenient basis for partition of all

physical phenomena, and may play the chief part in the physics of the future."

It is, however, unnecessary to give away the case for biology so easily. The great success of contemporary physics tends to obscure certain historical facts. Dalton's discovery of molecules antedated that of cells by Schleiden and Schwann by little more than twenty years. Theories of light, heat, and electricity have been about as long in the making as theories concerning the origin of species, the laws of heredity, and the nature of disease. Attaining clearness in any of them has not proceeded without much speculation, sore trial, and increasing insight under great difficulties. Considering the complexity of living matter, it is extraordinary that results so apparently reasonable should have followed so soon in the wake of discoveries concerning the behavior of non-living matter. In spite of the greater difficulty of generalization, of exact expression, and of mathematical statement in the case of living matter, effort in this direction will no doubt nevertheless go on. If certain data are inexpressible in mathematical terms a form of expression inherently suitable, but different from these, must be substituted. Accurate classification may serve instead, a procedure, however, much more extensive and complex as E. W. Hobson (*The Domain of Natural Science,* 1923) has pointed out than is that in sciences dealing with non-living matter. If classes can be well defined, thinking of adequate accuracy and usefulness may take place. The added need for great accuracy in definition is apparent. Under these circumstances the function of experiment is obvious and for this reason of first-rate importance. To bring precision into definition is no doubt the source of its great value. This is a subject on which Goethe, Galilei, and Bernard have things to say, different as I think from the current acceptance of its significance. Galilei, for instance, is reported by Fahie (Singer's *Studies in the History and Methods of Science,* Vol. II, p. 251) to have said "Ignorance had been the best teacher he ever had, since, in order to be able to demonstrate to his opponents the truth of his conclusions, he had been forced to prove them by a variety of experiments, though to satisfy his own mind alone he had never felt it necessary to make many."

These observations have a meaning for whatever arrangements are made for study and investigation. We are dealing with a disinterested science and with its application in practice. As in all such situations, there is the obligation to teach, the need to practice, and the opportunity to investigate. Of the need of practice and of teaching nothing need be said; they are well established academic activities. But the place of research is not so well defined. It is difficult to establish the truth of the statement, but there is a general

impression that the study of disease leads for the most part to a career only in the practice of medicine. That the phenomena of life exhibited by diseased cells may be investigated apart from this motive, that they can be studied as can any other biological system, is not a familiar belief. And yet there is no doubt that they lend themselves to this purpose. Disease is also a state of nature. The study of diseased systems may, indeed, yield information of first-rate importance concerning the behavior of living organisms. Both are natural and might for the purpose of biological generalization be equal. This is an idea which may very well become the basis of a conscious direction in the study of medicine of which until now no advantage or relatively little has been taken. The kind of addition to knowledge which may come from the pursuit of study of this sort is illustrated by reference to the title of such a work as Adami's *Medical Contributions to the Study of Evolution,* 1918. If it becomes recognized that the study of diseases offers these opportunities, medicine at once will be seen to take on new aspects. Men entering the study with widely different purposes will aid in the pursuit of its aim. It becomes unnecessary any longer to center interest in diseases exclusively from the point of view of the practice of medicine. So various, indeed, will become the opportunities for research that professors will develop with a range of interest widely divergent from those which are familiar. Recently the idea has been expressed that if the psychology of a man is known, you may know at once the type of his philosophy. And so in the walks of medicine. Men with different interests will be wanted, moreover, in order to fill all the necessary functions, men with varied capabilities, including the ability to teach, the ability to administer, and the ability to practice. In organizing our clinics it may be better to regard these functions separately and to make separate provision for each, coördinated perhaps, but not necessarily unified in a single individual. There is implied the consideration that in order that men of interests and abilities so widely different may become available to serve as leaders in these various directions, the opportunity for appropriate training for each calling must be arranged.

Views like these, separating but not stratifying the phases of medical interest, may be expected to free medicine so that it may flow unhindered in its several appointed ways. To fail to make this distinction is to fail to define precisely what the various functions of medicine (or any other science and its application) may be. Otherwise medicine will remain tied either to practice or to research. Of how this may happen we have had experience. At periods not so long past it was the custom, perhaps not universal, but nevertheless widely practiced, to restrict the budget and the opportunities of

professors of the so-called contributory or basic sciences to requirements commensurate with the interests of the practice of medicine. Such limitations proved to be intolerable. But this picture has a reverse. When the movement looking forward to the emancipation of medicine from the current needs of practice began, the opportunities of the clinics were reserved for men holding strictly academic posts. Here again there were introduced limitations which proved to be intolerable. Unless provision is made for the various functions implicit in medicine, medicine will remain, as I have said, tied either to practice or to research. The forward movement of the whole discipline will be hindered. If there is to be free development of all the functions of medicine, the impediments to growth should be removed. Define the phases and all who are concerned become free. On the basis of these considerations it becomes possible, perhaps for the first time, to describe without unfortunate reserve the several activities to which an interest in disease gives rise—to teach, to practice, and to investigate.

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BOOK REVIEWS

La Pensée d'après les Recherches Expérimentales de H.-J. Watt, de Messer et de Bühler. ALBERT BURLOUD. (Bibliothèque de Philosophie Contemporaine.) Paris: Félix Alcan. 1927. Pp. 192.

Intelligence and Mental Growth. C. A. CLAREMONT. New York: W. W. Norton & Company. 1928. Pp. 120.

Both of these volumes give evidence of serious study and careful workmanship within the limits set by the authors. But neither of them contains subject-matter that will appeal strongly to contemporary American psychologists. Dr. Burloud has delved into time-worn archives to review and evaluate, for French readers, the Würzburg studies on the higher thought processes conducted some twenty years ago. Mr. Claremont sets forth a speculative theory of the nature of intelligence.

The researches of Watt, Messer, and Bühler, it will be recalled, were designed to throw light upon what takes place when one is thinking, forming a judgment, arriving at an associative response, perceiving a meaning. The method of introspection was used, and a variety of simple tests were employed to provoke the mental activity that was being observed. The subjects were called upon not only to respond to the test items, but also to describe, as fully as possible, all the experiences that occurred in the interval during which the response was being made.

The results of these experiments, published in the *Archiv für die gesamte Psychologie* during the years 1902 to 1908, took form in a mass of introspective data, an elaborate dialectic bearing upon the findings, a good deal of controversial writing, and some general conclusions as to the nature of the higher thought processes. Burloud presents a faithful and well-written account of the entire enterprise so far as it relates to the three investigators mentioned in his title. A chapter is devoted to each one of these, and a final chapter gives a summary of the results and an appraisal of their value.

In Burloud's opinion, these researches offer no definite conclusions as to the construction of thought, nor any final answer as to the dynamics of mental activity. But he accords high praise to the Würzburg psychologists for the work they have done. According to him, their skillful use of the method of introspection, their profound observations, and their subtle analyses have revealed to psychology "new and marvellous horizons."

Twenty years ago this tribute might well have been received as a hopeful prophecy, but uttered at this late day it is somewhat lacking in force. To be sure, the investigation of mental processes and subjective events is still of major concern in the science of psychology. But the attack has taken a direction somewhat removed from the horizons revealed at Würzburg. On the one hand, there has been a tendency to decry what has been regarded as the naïve structuralism and futile dialectic of these earlier studies. Hard upon the investigations of Watt, Messer, and Bühler came the advent of behaviorism, the extreme antithesis of Würzburg, and violent in its criticism of introspective methods. On the other hand, sober psychologists have likewise moved away from the prevailing concepts of the Würzburg school; still perceiving in the method of introspection a valuable instrument of research, they have come to view the problem of thought from the standpoint of the formula of stimulus and response and the principle of redintegration. None the less, Burloud's book deserves recognition as a serious resumé of an historical period.

Another excursion into the analysis of mind, but somewhat more specialized in its purpose, is portrayed in Claremont's *Intelligence and Mental Growth.* In a brief preamble the author informs that his efforts are patterned on the methods of Fabre, Freud, and Montessori, to whom "three of the greatest advances in modern psychology" are due, and "whose disregard, almost disdain, for the ordinary technicalities of scientific statement, for the text-book preoccupation with definition and terminological precision, is almost proverbial."

Pursuant of his theme, Claremont presents the theorem that "the ultimate intelligence factor—that portion of the mental process which can not be classified in any other category, and therefore demands a separate name—is the power to become aware of the necessity in the very nature of things of certain causal relationships." This ultimate factor, he goes on to explain, does not exclude the necessity for training and experience, nor eliminate the possibility of individual differences. For it is "after a sufficient experience has occurred" that "a state of mind may be reached in which the sequence or coexistence noted is realized in some way as inevitable. This degree of understanding occurs in *some* cases of inevitable interconnection and not in others. It also occurs in *some* minds and not in others." The occurrence of this understanding of inevitable sequence he regards as the "single distinguishing mark of the 'intelligent mind.'"

According to Claremont, other processes besides the "perception of causation" (intelligence) participate in the evolution of a mental response. From a consideration of these, he derives a solution of the vexing problem of what is the "common factor" in all intellectual activity. Says he, "all the elements we have spoken of—associative recall, 'perception of causation,' creative power, judgments of similarity, perseverance, etc.—are themselves complex, and could be resolved into 'atomistic' elements, many of which would be common to all. *Hence these common elements take part in every single activity.* There is your common factor."

To describe intelligence as the "direct perception of causation," whereby, apparently, is implied a unique state of consciousness, is to offer a definition which itself needs defining. Claremont proceeds to prove his thesis by means of many illustrations, drawn from the fields of logic, geometry, physics, and from situations in every-day life. These illustrations, and the arguments that support them, are subjected to a good bit of interpretation and rationalization especially designed to vindicate the author's point of view. The book as a whole depicts an attempt to dress the concept of intelligence in a new terminological garb, and does not, in the opinion of this reviewer, add appreciably to one's understanding of the subject, by way either of theoretical insight or of concrete information.

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NOTES AND NEWS

There will be held in Paris in October of this year an International Congress of Applied Psychology. For information, address M. Paul Masson-Oursel, Directeur d'Etudes à l'Ecole des Hautes Etudes, Paris, France.